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OF THE

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OF

ELECTRICAL ENGINEERS.

CONTENTS.

PAGE.

Associate Members, elected March 23d, 1898.....185
Standardizing Committee.....186
Council Nominees.....186
Insulation and Conduction. (Illustrated.) By Reginald A. Fessenden, March 23d, 1898.....187
Discussion, March 23d, 1898.....219
Communication. By Reginald A. Fessenden.....223

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AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

New York, March 23d, 1898.

The 123d meeting of the INSTITUTE was held this date at 12 West 31st Street, and was called to order by President Crocker at 8:20 P. M.

THE PRESIDENT:—The Secretary has a few announcements to make before taking up the business of the evening.

THE SECRETARY:—At the meeting of the Council held this afternoon the following associate members were elected:

Name.	Address.	Endorsed by
DAVIS, ALBERT G.	Acting Manager, Patent Dep't, General Electric Co., Schenectady, N. Y.	Chas. P. Steinmetz. Ernest Berg. Eskil Berg.
GALLATIN, ALBERT R.	Student at Columbia University, residence 58 W. 55th St., New York City.	F. B. Crocker. W. H. Freedman. W. H. Ripley.
GAYTES, HERBERT	Electrical Engineer, Realty Syndicate Railways, Piedmont Power House, Oakland, Cal.	W. M. Stine. B. J. Arnold. S. A. Rhodes.
GRIFFIN, RUSSELL AGNEW.	Purchasing Agent, American Telephone & Telegraph Co., 15 Dey Street, New York City.	F. A. Pickernell. S. D. Field. R. W. Pope.
LEE, FRANCIS VALENTINE	T. Engineer, (Pacific Coast Dept.) Stanley Electric M'f'g Co., 800 California St., San Francisco, Cal.	F. A. C. Perrine. C. L. Cory. F. P. Medina.
LOEWENTHAL, MAX	Associate Editor, *The Electrical Engineer*; residence, 831 Park Ave., New York City.	F. B. Crocker. T. C. Martin. Max Osterberg.
POPE, HENRY W.	Special Agent, American Telephone & Telegraph Co., residence, 200 W. 83d Street, New York City.	S. D. Field. R. W. Pope. T. D. Lockwood.
REICHMANN, FRITZ	Instructor of Physics, The University of Texas; 309 E. 11th St., Austin, Tex.	A. L. McRae. H. H. Humphrey. Brown Ayres.
THEBERATH, THEODORE E.	Pacific Coast Engineer, Stanley Electric M'f'g Co., 800 California St., San Francisco, Cal.	F. A. C. Perrine. Geo P. Low. F. P. Medina.

FLIESS, ROBERT ANTON,	Student of Electrical Engineering, Columbia University; residence, 201 W. 55th St., New York City.	F. B. Crocker. G. F. Sever. W. H. Freedman.

Total 10.

In accordance with the wishes of the meeting which was held at New York and Chicago to discuss the question of standardizing generators, motors and transformers, the Council this afternoon appointed the following committee of seven to consider the question and report to the Council:

FRANCIS B. CROCKER, J. W. LIEB, JR.,
CARY T. HUTCHINSON, C. P. STEINMETZ,
ARTHUR E. KENNELLY, L. B. STILLWELL,
ELIHU THOMSON.

At the same meeting, in accordance with the Constitution, the Council selected the following nominees for the coming election from the nominations sent in by the membership:

For President:
ARTHUR E. KENNELLY.

For Vice-Presidents:
ROBERT B. OWENS,
WILLIAM STANLEY,
CARY T. HUTCHINSON.

For Managers:
HERBERT LLOYD,
SAMUEL SHELDON,
GEORGE F. SEVER,
CHARLES P. STEINMETZ.

For Secretary:
RALPH W. POPE.

For Treasurer:
GEORGE A. HAMILTON.

The nominations sent in very clearly expressed the desire of the membership that the present incumbent should serve for another term as President, but Dr. Crocker positively declined, on account of his probable absence during the next year, beginning in the fall. He expects to go abroad and he wished to cut loose from all duties, of which the presidency was one. The paper this evening was to have been read by the author, but I received from him this morning a letter explaining his unavoidable absence.

THE PRESIDENT:—It is quite unfortunate that Prof. Fessenden cannot be here to present his paper, as it is an interesting subject. The paper was printed in advance and has been accessible to some of the members. But as most of the members have not had an opportunity to read it and as the author is not here to give us an abstract, it would be well to have the paper read and Mr. Ryan has kindly offered to do this.

A paper presented at the 137rd Meeting of the American Institute of Electrical Engineers, New York, March 23rd, 1898, President Crocker in the Chair.

INSULATION AND CONDUCTION.

BY REGINALD A. FESSENDEN.

A thing insulates because it is possessed of two distinct properties, first, the ability to stand the mechanical and electrical stresses due to the voltages used; and, secondly, because it is such a poor conductor that but a negligible small current can flow through it and leak away. In other words, it will neither allow the current to break through it, nor to steal through it. The first property is called by Maxwell the "dielectric strength" of the insulator, the other property is called the ohmic resistance. The two together form its insulating power.

In the two great branches of electrical work, the requirements for an insulator are widely different. For apparatus used for the transmission of intelligence as a rule low voltages are used, and so dielectric strength is of relatively small importance, but the currents used are small, the circuits long, and material of high ohmic resistance is needed. For apparatus designed for the generation and transmission of electric energy, on the other hand, where the voltages are high and the currents large, dielectric strength is the main thing desired and the leakage of a small amount of current is not objectionable. Consequently the two branches of the profession have come to use the word "insulation" in quite different senses, the former and older as meaning something having high ohmic resistance, and the latter branch using it with reference chiefly to material having great dielectric strength.

Confusion sometimes occurs through this double meaning, and the writer himself has been taken to task by a European engineer for stating that pure water was approximately as good an insulator as rubber, the critic having reference, as was apparent,

to its ohmic resistance, whilst the original note was principally concerned with its dielectric strength. It is therefore considered best to define the sense in which the word is used, in spite of the fact that attention has previously been called to the distinction to be made between these two properties; notably and very lucidly by Mr. Steinmeitz in this INSTITUTE'S proceedings, vol. ix., p. 815.

Before entering upon the discussion of the various purposes for which insulation is used and of the substances best suited to each case it may be as well to give a brief account of the manner in which the current passes through materials.

1. *By actual convection.*—That is by particles of the insulator, or of foreign substances, taking a charge from one electrode and moving with it to the other terminal under the influence of the voltage. This action is similar to that of the moving pith ball between the two knobs of a Holtż machine.

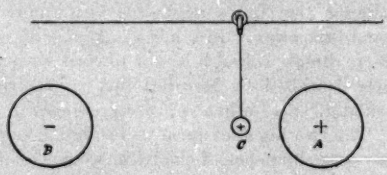

FIG. 1.

The ball C having touched A and got a + charge, is repelled and moves over to B, where it gives up its charge, takes up a — charge and moves back to A.

This phenomenon is not known to take place in solids, but it is quite marked in gases, vapors and fluids.

Until recently it was a question of no practical importance in electrical engineering, but with the high voltages now in use or contemplated it may give serious trouble, and apparatus must be designed to check this form of leakage. It was not so long ago that Ferranti's 10,000 volts was looked upon as monstrous, but some experiments of the large companies, of which I have been informed, seem to show that 100,000 volts may be quite practicable, even in quite unfavorable climates. With overhead wire the leakage will merely mean a loss of energy, and the use of porcelain for all insulation, as any oxidizible material (used for instance to protect the primaries of transformers), would be

speedily destroyed by the ozone. Where, however, oil is used in the transformers, the leakage may cause quite serious trouble, owing to the large surfaces and their proximity. In addition, as Prof. Elihu Thomson pointed out some years ago, with high potentials, impurities in the oil are apt to group themselves along the lines of highest slope of potential like the iron filings around a magnet, and if, as is generally the case, the impurities have a higher specific inductive capacity than the oil such a group is thus apt to form a bridge between two points of great difference in potential hence causing an arc. In those convection currents it is not, I believe, the very small particles which cause the trouble, because small bodies, when charged from a comparatively large and smooth one are not repelled but attracted; consequently a small grain of dust after touching a highly charged flat conductor would remain close to it if there were no negatively charged particles near it to drag it away or no currents in the oil to wash it off.

The relation between the size of the particle, the voltage and the radius of the charged conductor when the particle after touching the conductor is neither attracted nor repelled can be obtained by the method of images, but the formula so derived is rather long and complicated and I have not had time to work out the numerical results.

It is evident, however, from it, that for a convective current to take place the radius of the particles carrying the discharge must be a quite appreciable fraction of the radius of the charged conductor. Consequently this form of leakage is due to the motion of a portion of the oil as a whole and not of its individual particles, and if we can break up mechanically these currents we can to a great extent stop the leakage from this cause. This may be done in three ways:—

1. By using oil of great viscosity, in which case, however, we lose the chief advantage of oil insulation, *i.e.*, its ability to reinsulate quickly after a discharge.

2. By putting pure dry cellulose in some form or other between the charged surfaces loosely, so that the oil can filter through it easily and any air escape readily, but sufficiently close to prevent any rapid flow. Pure cellulose has the great advantage that when well boiled in the oil it has approximately the same specific conductive capacity as the oil. No varnish or shellac should be used in the oil for reasons given later.

3. By dissolving a solid, non-disassociating substance in the oil in such excess that it crystallizes out at ordinary temperatures and forms with the oil a soft gelatinous mass, not fluid, but yet capable of allowing the oil to ooze through its substance. This has many of the disadvantages of 1, but it has one advantage, in that the substance chosen may be one, like paraffin, having a large specific heat of liquefaction, and consequently an overload will not raise the temperature of the oil above a fixed point till the paraffin is all melted.

The effect of points in promoting convective discharges in air is well known. It is usually attributed to the great surface density of electricity which a point must take in order to make the potential all over the conductor the same, and hence, since the repulsive force varies as the square of its surface density, it is evident that there will be a great tendency for discharge from a point from this cause. But there is another and very important one, *i.e.*, the fact that, as mentioned above, a particle cannot take a charge and move away unless its radius is larger than a certain fraction of the radius of the curvature of the conductor at the point where it touches the latter; consequently when the charged surface is a plane only large aggregations of atoms can move away; these move slowly and carry small charges in proportion to their mass. But at points where the radius of curvature is very small, small particles can move away with great rapidity and with relatively large charges. Rounding off or flattening the charged surfaces thus acts in a double way, by reducing the surface density and by preventing all but large sized particles moving away.

2. *Conduction in Solids.*—It is not absolutely certain that all conduction is not by convection, but the terms are here used with their usual signification. In solids we do not know as yet exactly how the discharge is handed on, but I have noted a very remarkable fact, which is quite significant and suggestive, *i.e.*, that the conductivities of metals are proportional to the quantity $\sqrt{\frac{\text{elasticity}}{\text{density}}} \div$ valency. The following table shows this.

This fact was discovered by the writer in 1892,[1] as the result of several years tedious work in collecting physical data and combining them into formulæ to see if any law could be found.

1. *Science*, July 22, 1892.

TABLE OF CALCULATED AND OBSERVED RESISTIVITIES.

METAL.	ATOM. VOL.	ATOM. WT.	VALENCY.	R. CALC.	R. OBSERVED.
Silver	10.2	108.	1	100	100
Copper	7.1	63.3	2	126	106
Gold	10.2	197.	1	135	137
Aluminium	10.5	27.	3	152	159
Magnesium	14.	24.4	4	290	275
Zinc	9.4	65.5	4	300	352
Cadmium	13.	112.2	4	456	450
Tin	16.2	118.	8	1030	878
Thallium	17.3	204.	6	1060	1190
Lead	18.2	207.	8	1470	1305
Iron	7.2	56.	8	480	646
Beryllium	2.	9.	4 or 8	50 or 100	....

Out of the hundreds of combinations tried, this and another one (really the same ultimately, but expressed in terms of other properties), were the only ones which seemed hopeful. It was stated in the article referred to, that this formula could not be quite correct. This was for the following reason: Silver, gold and aluminium should, as will be seen from the formula given below, have resistances proportional to the square roots of their densities multiplied by their valencies, *i.e.*, in the ratio of

$$\sqrt{10.6} \ : \ \sqrt{19.26} \ : \ \sqrt{2.65} \times 3$$

i. e.,

$$100 \ : \ 136 \ : \ 150.$$

Now at this time the best determinations of the resistance of aluminium with which I was acquainted gave it as 193 to silver 100. With such a wide discrepancy therefore between calculation and observation, *i.e.*, 193 instead of 150, it did not seem probable that the high observed value could be modified by subsequent determinations so as to agree with the calculated one. It was, therefore, with considerable pleasure that I saw the recent determination of Messrs. Richards and Thomson (published last year in the *Journal of the Franklin Institute*). Their results for aluminium, 99.66 pure, were:

$$\text{Aluminium} \ : \ \text{Silver} \ : : \ 163 \ : \ 100,$$

and they expressed the opinion that the value for pure aluminium,

when hard-drawn, would be 66 per cent. of the conductivity of copper. This result is so close to the calculated result given in the paper referred to, *i.e.*,

$$17 : 27 = 100 : 159,$$

though such a high conductivity for aluminium was considered beyond the bounds of probability at that time, that I feel justified in considering that the formula given may be found fairly accurate when more exact determinations shall have been made.

This formula throws a certain light on the nature of conductivity in solids, and why some solids are insulators. For the formula $\sqrt{\frac{\text{elasticity}}{\text{density}}}$ is the same as that for the velocity of sound in a body. Now, in the convective discharge, the electricity was handed on with the same velocity as that with which the particles moved. In fluids, as we shall see, the electricity is handed on with the velocity with which the ions move. In both cases the electricity travels along on the particles of matter.

The idea that electrical flow in solid conductors might also be a simple handing on from one atom to another suggested itself nearly a decade ago to Professor Lodge. That brilliant and careful reasoner and experimenter, who has cleared up so many patches of scientific jungle, gives a very clear description of the manner in which this would happen if such were the case. I cannot do better than quote him, as it will show how well this idea agrees with all the facts then known, but one.

"But if we are not satisfied with this vague analogy, and wish to penetrate into the ultimate nature of heat and the mode in which it can be generated, then we can return to the consideration of a multitude of oscillating and colliding particles, moving with a certain average energy which determines what we call the temperature of the body. If now one or more of these bodies receives a knock, the energy of the blow is speedily shared among all the others, and they all begin to move rather more energetically than before: the body which the assemblage of particles constitutes is said to have "risen in temperature." This illustrates the production of heat by a blow or other mechanical means. But now, instead of *striking* one of the balls give it an electric charge; or, better still, put within its reach a constant reservoir of electricity from which it can receive a charge every time it strikes it, and at the same time put within the reach of some other of the assemblage of particles another reservoir of infinite capacity which shall be able to drain away all the elec-

tricity it may receive. In practice there is no need of infinite reservoirs: all that is wanted is to connect two finite reservoirs, or "electrodes," as one might now call them, with some constant means of propelling electricity from one to the other, *i.e.* with the poles of a voltaic battery or a Holtz machine.

What will be the result of thus passing a series of electric charges through the assemblage of particles? Plainly the act of receiving a charge and passing it on will tend to increase the original motion of each particle; it will tend to raise the temperature of the body. In this way, therefore, it is possible to picture the mode in which an electric current generates heat.

But although this process may be used as a possible analogy, it cannot be a true and complete statement of what occurs; for it is essentially the mode of propagation of *sound*. Sound travels at a definite and known velocity, being a mechanical disturbance handed on from particle to particle in the manner described. But heat, being some mode of motion, must also be handed on after some analogous fashion, so that when heat is supplied to one point of a mass it spreads or diffuses through it. It is difficult to suppose the conduction of heat to be other than the handing on of molecular quiverings from one to another, and yet it takes place according to laws altogether different from those of the propagation of the gross disturbance called sound. The exact mode of conduction of heat is unknown, but, whatever it is, it can hardly be doubted that the conduction of electricity through metals is not very unlike it, for the two processes are the same laws of propagation: they are both of the nature of a diffusion, they both obey Ohm's law, and a metal which conducts heat well, conducts electricity well also."

I have said, "with all the facts then known but one." This because, as mentioned in the abstract given above, there seemed to be no good evidence for the view that there was any connection between the conduction of sound and of electricity. The reason for this lay in the fact that very few determinations had been made of the velocity of sound in the pure metals, though a considerable number had been made on alloys and commercial materials.[1]

1. I am here moved to again call attention to the fact that an immense amount of physical experimental work is misdirected. It is no doubt quite gratifying to ascertain, after several years laborious work, that a particular piece of brass or steel of not very definitely known composition and in a quite indefinite physical state has a temperature coefficient of expansion expressed by five significant figures, but such information must be considered as pieces of philosophic virtú intrinsically worthless, but possibly possessing a value by reason of their uniqueness and associations. I have pointed out elsewhere (*Jour. Frank. Inst.*) that at the present time, in spite of the fact that much has been done, notably by

Consequently no general law could be discovered, though it cannot be doubted but that had the formula for the relation between Young's modulus and atomic volume discovered by the writer[1] been known at that time, this evidence of a still more intimate connection between sound, heat and electricity would have been discovered by Dr. Lodge.

The formula referred to is:

$$\text{Young's Modulus} = \frac{78 \times 10^{12}}{(\text{atom. vol.})^3}.$$

Hence it is possible to predetermine the velocity of sound in wires by the formula:

$$\text{Velocity in cms. per sec.} = \frac{883 \times 10^4}{\text{atom. vol.} \times \sqrt{\text{density}}},$$

and the electric resistivity is given roughly by:

$$\text{Resistivity} = 45 \times 10^{-9} \times \text{atom. vol.} \times \sqrt{\text{density}} \times \text{valency}.$$

This formula possesses a general interest, inasmuch as it would seem that while the strain in the dielectric is propagated with the velocity of light, *i. e.* $\sqrt{k\mu}$, the actual electricity in the wire is handed on with the velocity of sound, and is proportional to $\sqrt{\frac{k}{\mu}}$. The significance of this will be treated of elsewhere.

Suppose a wheel, with elastic spokes and a heavy rim bound outside with a band of horsehair, the horsehair rubbing against a series of violin strings mounted parallel to the axis of the wheel, as shown in Fig. 2. Suppose each violin string has mounted upon it a small metallic bead charged with electricity, so that when the strings vibrate the beads can touch. Then on grasping the wheel at B, after a time depending upon the elasticity and mass of the *wheel*, a spark will be seen at A, but the actual velocity with which the electricity moves round the circuit of violin strings will depend upon the elasticity and mass of the *strings*. In passing, we may note several things:

Matthiessen, Roberts-Austen and others, we have not at present any data in regard to 90 per cent. of the more important properties of the simpler metals used in the arts. The importance of having a standard state for solids; of a central bureau to furnish pure materials in a standard physical state to experimenters, and the exclusive use of such materials by experimenters, cannot be overestimated.

1. *Elec. World*, Aug. 22, 1891; *Science*, July 22, 1892.

1st. That for this analogy to hold at all, the atoms must be charged even in a conductor not carrying current; which is significant when taken in connection with the fact pointed out by the writer in the papers above referred to, that if we calculate the tensile strength, rigidity and Young's modulus on the assumption that these effects are due to charges on the atoms, we get results agreeing very closely with experiment.

2nd. That in a circuit having many atoms in the cross-section the energy could be handed on without actual contact by electrodynamic processes.

3d. That if one part of the circuit were composed of violin strings tuned to a different note-period from the other we would get effects similar to those of thermo-electricity.[1]

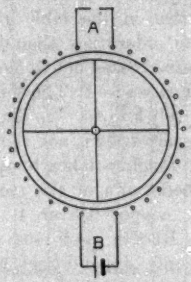

FIG. 2.

4th. That the reversal of the point of greatest drop of potential in a circuit of air and carbon from the + to the — electrode as the pressure is reduced may be due to the fact that, as shown by the beautiful experiments of J. J. Thomson, the molecular conductivity of air at low pressures is extremely high, the electrode heated thus depending upon whether the solid conducts better than the gas, or vice versa.

5th. That this same formula would also be applicable to a diffusion phenomenon, and indeed the presence of valency in the formula makes it exactly analogous to that for the diffusion of heat. Moreover, Roberts-Austen, in a series of striking experi-

1. Compare the analogous problem of two portions of gas of different densities in a closed ring tube, and junctions heated. Neuman, Jahrberichte f. Chem., 1874, p. 15.

ments, has shown that diffusion can take place in solids at ordinary temperatures. If a current be however a diffusion, it may be shown that for fairly large current densities the atoms must move several cms. per second and we might therefore expect interpenetration when brass and zinc terminals are joined. But this does not occur to any considerable extent. We may also take it as an electrolytic diffusion, as is suggested by J. J. Thomson in his "Dynamics Applied to Physics and Chemistry," but in this case, even assuming the theory suggested by him to account for some atoms preferring a plus to a negative charge, I find it difficult to imagine the manner in which the atoms could be charged and discharged at the surface of the conductor. It is to be noted that in gases the phenomenon of diffusion, sound velocity and heat conductivity are all linked together.

It will be noticed that in the table given, while the metals follow each other in the order they should from calculation, yet the agreement is in some cases not so close as in others. This may arise from several causes:—1. The conductivities of but few of the metals are accurately known. 2. The elasticities of but few of the metals are accurately known, and the formula given for the Young's modulus only roughly takes temperature into account. 3. The temperature coefficients of the metals are not the same. For instance, that for copper is over .4 : .415, having been obtained by Swan and Rhodin, and .404 by Kennelly and the writer, whilst silver is only .38, and gold .36. Consequently the ratio will vary with the temperature, and at higher temperatures the ratio, resistivity of copper : resistivity of silver will be less. This, it is interesting to note, holds true for the velocity of sound, for from Wertheim's results we have, velocity of sound in silver : velocity in copper = 1.35 at 20° C., and only 1.19 at 200° C., and hence, just as in the case of electrical conductivity, the sound conductivity of copper diminishes at a faster rate than that of silver.

The magnetic metals, as iron and platinum, are very difficult to obtain pure. Their true resistance is therefore at present doubtful.

What has been mentioned thus far concerning the resistance of solids has had no immediate bearing on the subject of insulation. It was necessary, however, in order to introduce the following considerations.

In the formulæ, the valency was introduced. This term has no definite meaning, as the valency varies with the compound. Consider the writer's chart of the elements, (Fig. 3), a modification of those of Meyer, Newlands and others. On one side are seen the metals of the arts. Above them in vertical rows are marked the figures 1, 2, 3, 4, etc. These figures indicate the number of chemical linkages which as a general rule the elements under the figures tend to take up. But there is no very definite

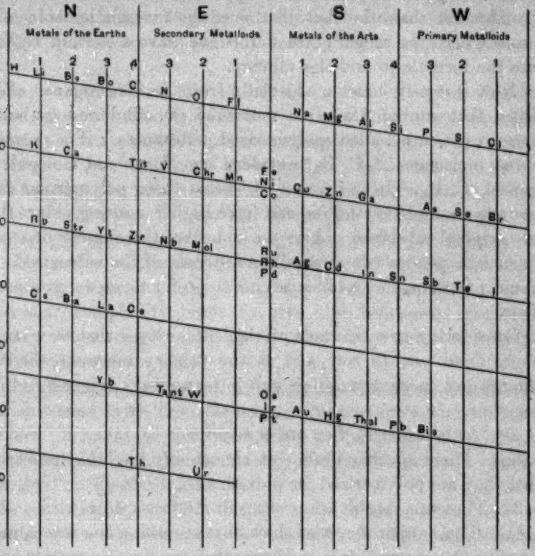

FIG. 3.

rule. Among the univalent metals some unite with bivalent atoms, as does copper, and in general all that can be said is, that a certain valency holds generally and not in general. Consequently when we find that by taking a group of metals having very closely the same values of Young's modulus, as for instance, gold, silver and aluminium, their conductivities are, within the limits of errors of observation, proportional to the velocity of sound ÷ valency; and that in any of the group of metals having

the same valency the conductivity is directly proportional to the velocity of sound, within experimental errors, we are to a certain extent justified in making a choice of valencies when this is needed. Fortunately, however, this only occurs in two cases: 1st, we must suppose copper to have twice the valency of silver, which we might, *a priori*, have granted, for though they are in the same group yet copper has markedly through all its salts, twice the valency that silver has. 2nd, that thallium has twice the valency of aluminium, a supposition which has no other justification than the fact that since the formula holds in other cases where the valency are known we have a certain right to use the formula to find the valency.

Now it is well known, especially from facts in organic chemistry, that similar atoms or molecules can combine together to form what are called polymerized substances. For instance three molecules of $C_2 H_2$ acetylene can form one molecule of benzol, $C_6 H_6$. In general we find that these polymerized compounds are more crystalline and have higher melting points than the original substance. Also we note that the number of atoms in such a polymer is generally a multiple of the valency of the atom. Carbon, for instance, seems to prefer to go in groups of four.

Considering now the table of elements, we see that as we pass along from row to row, and as the valency increases, the substances get more crystalline and in many ways evince a linking together.

1st. As mentioned, they are crystalline.

2nd. Their specific heats get abnormally low, this indicating that they are polymerized, or plexed.

3d. They are capable of existing in allotropic forms.

4th. Their vapor densities show that several atoms are jointed together into one molecule, and hence they are almost certainly polymerized as solids since the general tendency of heat is to disassociate. Consequently there is some evidence for the theory that metalloids differ only from metals in that they have a greater tendency to polymerize from their higher valencies, which are probably in some way dependent upon the shapes of the atoms, and are so more crystalline, etc. We might therefore look for some evidence of polymerization among the metals. This is readily found, as before from the variation of the specific heat from its theoretical value. If this linkage were but loose, it is evident that it

might affect the specific heat but little, and yet have a marked effect on other phenomena. For instance, we might suppose that it would affect long sound waves less than short ones.[1]

We might therefore consider that when the period of a sound wave coincides with that of the molecules, it is a heat wave, and that heat is transmitted with the velocity of short sound waves, but with a large logarithmic decrement. If this be proven by experiment we would have this relation; *The electric conductivities are in the same ratio as the velocities of very short sound waves*, being thus analogous to Maxwell's law for the velocity of light in insulators, in that the velocity varies with the periodicity.

We have seen above that the electric resistivity varies directly as the valency. Also it has been indicated that there is some evidence to show that the polymerization or plex varies as the valency. Therefore we are led to assume that increase of resistance accompanies polymerization or the linking together of the atoms into groups. We see that this might hinder the transmission in various ways since the groups would not vibrate so quickly as the single atoms and there would be fewer of them per unit cross-section. Anything, therefore, which tends to give molecular complexity, tends to give high resistance; hence we see why alloys are generally higher in resistance than the average of their components.

The resistance increases with density and molecular complexity and inversely with the elasticity. Consequently whether a resistance increases or decreases with temperature will depend upon whether the molecular union is weakened at a less or greater rate than the elasticity falls off.

In getting a concept of this we may consider a conductor as analogous to a government department, the different atoms or officials being bound together into groups by valency bonds, the analogue to which is evidently "red tape." The rate at which a given impulse is handed on will evidently depend inversely upon the amount of red tape and the density, or stupidity, of the individual official. The two causes are often confused.

1. The writer ventures to suggest that the velocity of very short waves of sound in copper may be found somewhat less than half of that given now for audible tones, while that of lead may be reduced to but one-eighth of its value as given at present.

We must therefore, for solid insulation, get substances which are strongly linked together, of great density, and of small modulus of elasticity. The first is the property which varies most, since density and elasticity vary between comparatively narrow limits.

3. *Conductivity in fluids.*—The nature of the manner in which electricity is conducted in electrolytes has been very thoroughly worked out by Clausius, Arrhenius, Hittorff, Kohlrausch, Nernst, Ostwald and others, and the work of these physicists has led to the linking together of results in a way which is simply marvelous. As the results can be obtained from works on physical chemistry, a very brief resumé is given.

The atoms of a solid are held together by the force of cohesion and driven apart by the hitting of the atoms on each other, due to the fact that they are in vibration, possessing kinetic energy proportional to the temperature. The molecule may also be pulled apart by the cohesive attractions of its atoms for other atoms. Whether a substance is a solid or a gas depends upon whether the fraction:

$$\frac{\text{Cohesive force of atoms for one another} + \text{external force}}{\text{Kinetic repulsion} + \text{cohesive attraction for other atoms}}$$

is greater or less than unity. We can thus turn a substance into a gas in two ways, *i. e.* by increasing the kinetic energy of the atom by heating it, or by bringing it in contact with other atoms, when, if the sum of the terms in the denominator is greater than the numerator, it will dissolve.

We have to distinguish two kinds of linkages in solids, the cohesive force of the atoms for one another in uniting to form a molecule, and the attraction of the molecules for each other. The former generally is stronger than the latter; consequently we may have a substance which on being put into contact with a solvent will have its molecules pulled apart from one another but not its atoms. In the presence of another solvent however, the second term may be sufficiently great to pull apart not only the molecules but also the atoms of the molecules. The substance is then said to be not only dissolved but disassociated. But dissolved substances give an osmotic suction per sq. cm. which is equal numerically to the kinetic pressure which the substance would have if it were turned into a gas at the same temperature and volume as the solution. A number of proofs of more or less validity have been given for this, but it seems to the writer to

follow at once from the obvious fact that if we take a solid and heat it and dissolve it, the kinetic repulsion must always equal the cohesive attraction plus the vapor pressure when equilibrium is reached.

This phenomenon of osmosis has been generally treated of as due to pressure and the dissolved substance to exercise a pressure equal to that which it would have if turned into a gas at the same temperature and volume. The writer (in the *Elec. Review*, London, Nov. 27, 1891) pointed out, as above, that the results were better explained by supposing that the solvent took up the cohesion of the solute, and that this got rid of the great difficulty of the disassociation theory, *i.e.*, that solution was generally accompanied by heating. Recently this theory has been put forward by other well-known physicists. Prof. Poynting (*Phil. Mag.*, Oct., 1896) has treated the subject mathematically, and whilst the mathematical reasoning cannot be considered conclusive, as Prof. Poynting himself states, yet, as he puts it, it shows "that it is not necessary to ascribe osmotic pressure to disassociation, but rather to association, or some kind of combination of salt and solvent." I have ventured therefore, in view of this fact, to do what I would not have done otherwise, *i. e.* to substitute my own conception of a suction for that of a pressure, otherwise making no change.

In this way by measuring the osmotic suction we can tell whether a salt is disassociated or not; and it is found that only those salts which are disassociated can conduct electricity. The molecules split up generally into two parts, one charged with positive, the other with negative electricity. These charged parts or ions when placed in an electro-static field, move with a velocity proportional to the slope of potential and to the specific ability of each ion to move among the crowd of molecules of the solvent. Consequently the faster an ion can get along through the crowd of other molecules, *i. e.* the faster it can diffuse, the faster will the electricity be carried, and the greater the amount carried per second for a given slope of potential; also the greater the quantity of electricity carried per ion the greater the current. The total quantity carried will be the sum of that carried by each ion, so that by adding the velocities of the ions we get the total velocity with which the electricity is moving.

The conductivity of a solution is thus dependent upon the following:

1. How powerful the attraction of the molecules of the solvent is for the ions of the solute, for on this depends how much of the solute is disassociated, *i. e.*, how many ions are set free to carry the current.

2. How fast the ions move.

3. What the valency of the ions is.

In designing insulations, the first is the important point. For from it we see that two good insulators mixed do not necessarily make a good insulator. A solid may dissolve in one substance and be an insulator in solution, but in another solvent may conduct quite well.

This is what makes the chief difference between fluid insulators, for practically all the fluids which are not simple elements, like mercury, have very high ohmic resistance, and all have practically about the same dielectric strength. The ohmic resistance of pure water is, according to Kohlrausch and Heydweiller, about one meghom per cubic centimetre, consequently on account of its non-inflammability and great specific heat, its great heat of vaporization and low boiling point, it would be a very valuable insulator for some types of apparatus were it not for the fact that it dissolves almost everything in slight proportions and splits them up into ions. Varnished paper will dissolve in some high resistance oils, forming a conducting solution.

C. *Conductivity in Gases.*—There is much evidence to show that conduction in gases is electrolytic, more especially J. J. Thomson's beautiful work on this subject. Also the fact that in air or CO_2 carbon is deposited on the negative pole, sometimes to the thickness of more than an inch in enclosed arc lamps, according to Marks, while in hydrogen or hydro-carbons in many instances the carbon is deposited on the positive carbon, would seem to favor this view. Also in its favor is Prof. Elihu Thomson's observation of the formation of copper "trees" in incandescent lamps with coppered filiament joints. Against this is Schuster's observation that the metallic lines in the spectrum have a fairly high velocity, and also the general appearances of the arc, which looks as if something were going in one direction down its centre and something else back on the outside. Also there is one other phenomenon which looks as if it were convective. This is the fact that if we take a solid carbon and bring it nearer to a similar carbon while the current is passing, the resistance first decreases and then increases. This was I believe first pointed out by Mrs. Ayrton

in her admirable paper on the subject. The reason of it has not I believe been given; it seems to be due to a necessity for circulation in the arc. If we have two carbons as in Fig. 4, at 1, the centre part of the current seems to flow all right, but the part B cannot flow unless the + carbon is very hot, and this is only cured by either increasing the current so as to heat the whole carbon up or by reducing the cross-section, as at 2 in Fig. 4. Whether a carbon is cored or not has nothing to do with this increase of resistance on the distance being shortened, as all that coring does is to diminish the cross-section of the carbon, and I have repeatedly found by actual test that the difficulty is entirely obviated by using carbons of × or — cross-section, provided the greatest thickness at any point of the cross-section is less than that of the cored. The phenomenon is evidently analogous to the jumping back of the discharge in a Geissler tube when the electrodes are brought too close together. The theory of this was given by

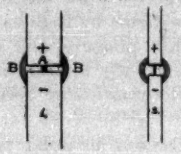

Fig. 4.

Fig. 5.

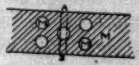

Fig. 6.

J. J. Thomson, and it was by applying this that I got over the difficulty with the carbons. A true arc can be run from a much lower voltage in open air than is generally supposed when the section of the carbon at any point is not more than $\frac{1}{100}$ cm. thick. On the whole, the evidence seems to be in favor of the belief that both convective and electrolytic discharges take place in air. The whole subject is very fully treated in J. J. Thomson's "Recent Researches in Electricity and Magnetism." There can be, in my opinion, very little doubt but that when a true electrolytic discharge takes place, as it does in a hot flame, the conductivity is proportional to the velocity of sound in the gas, though so far I am not aware that any experiments have been made on the subject. It is possible that in certain cases the elasticity itself may be a function of the slope in the electrostatic field. As the vacuum increases up to a certain point, the dielectric strength decreases, and this point depends somewhat on the electrodes and

voltages, and has led to the amusing result, that on the average every two years the discovery is announced that a vacuum is a good conductor. Above this, the discharge appears to get more and more convective, and a fact may be stated which I have not seen mentioned, *i. e.*, that the largest quantity of X-rays is not got from a tube unless the path of the positive particles back to the cathode is blocked up as much as possible. An experimental tube made by Mr. Meadowcroft for the writer last spring, in which there is a small tube running from the back of the anode to the back of the cathode which can be blocked up by tilting it, shows this very nicely.

As regards the electrolytic discharge, this can only take place when the gas is disassociated by heat or by a strong slope of potential. To the convective discharge the same remarks apply as to the convective discharge in gases. At ordinary pressures the only gases which allow a discharge to pass easily are helium, argon and possibly that unknown gas whose spectrum we see in the light of the Aurora Borealis. Helium, as shown by Ramsey, behaves at ordinary pressure much as air at low pressure, and a spark will jump through it for about thirty times the distance it will through air at the same pressure, when the pressure of both approximates that of the atmosphere.

Having got rid of the theory which was necessary in order to give opportunity to condense later, and which has led me to wonder if it were ever possible to find the pleasing mean between the conciseness of the Carpenter and the discursiveness of the Walrus, we shall take up the practical part of the subject.

High Resistance Insulation.

In laboratory apparatus in many cases, for instance with electrometers and resistance boxes, we need as high ohmic resistance as it is possible to get. Here however we are met by the fact that the two substances most commonly used, *i. e.* hard rubber and glass, are among the poorest insulators known for this class of work.

Rubber is very objectionable from the fact that whilst it presents a nice bright appearance when new, it contains sulphur and is very easily oxidized, especially when exposed to light. A film of sulphuric acid is thus formed on the surface, and if the tongue be applied to a piece of rubber which has been in use for some time the taste of the acid is very strong. I have seen the top of

a Wheatstone bridge, supposed to be capable of measuring accurately to one part in 5000, in which the total length exposed to leakage, divided by the average distance between which leakage could take place and the average voltage was only .008, with the top so acid that the tongue could hardly be allowed to touch it.

As a rule it is very hard to remedy this; rubbing the surface does no good as the acid extends in to some distance. Rubbing with cigar ashes is advocated by some, but I should fancy it would be almost impossible to remove the last traces of alkali. The method used by the writer is to steep the rubber in warm 10 per cent. caustic soda, then in warm distilled water, frequently renewed, then drying in the dark quickly and rubbing with pure paraffin, treated as described under paraffin, then polished while warm. This does good for a time, until the paraffin takes up dust.

For rods, a good way is to treat as above and coat half an inch thick with paraffin; then run over the rod with a wooden die and cut a thread in the paraffin. Run over the thread about once a month, and good results will be obtained.

With bridges, however, it is impossible to remove the top, and the only thing which can be done is to keep them covered up from light.

Rubber has also one other disadvantage, in that it does not show dirt, and where rubber comes in contact with copper it is apt to rot.

Glass is very bad because the alkali in it has a great affinity for moisture. The alkali is slightly soluble, and hence it is the custom with analytical chemists to boil all beakers used in exact work for several days before using, so as to get the soluble alkali and silica out of them. When possible this should be done with the glass of electrical apparatus. Another very serious trouble is that the angle of contact between water and glass is zero, so that when a drop of water is placed in the middle of a pane of clean glass it immediately spreads all over it in a thin film. This method is used by chemists to determine when a glass is clean. Nothing much can be done with glass but to keep it dry. Sulphuric acid is generally used, but it sometimes, if allowed to get dust in it, gives off vapors which condense on the sides of the apparatus. This, however, does not often happen.

Evidently we need some substance of high ohmic resistance and one which water will not wet. Boys, who has earned the thanks of electricians for his happy discovery of an almost perfectly elastic fibre, has given us also, as he himself has pointed out, such an insulator in quartz. Dip a thread of glass in water and lay it between the knob of a charged electrometer and the ground, and the leaves close almost at once, the whole fibre being covered with a film of water. Treat a quartz fibre similarly and the water slides off it, or remains in alternate big and little drops, each separate from its fellow, and the insulator is apparently as good as the air itself.

Quartz should therefore be used as much as possible in electrical instrument work. It can be melted in a powerful gas flame furnace and though it can never be melted down free from small bubbles, these make no difference except in appearance. It is however, possible to obtain glass which contains no alkali, and resembles quartz in that it is not wet by water. Such a substance is Faraday's borate of lead glass, as he himself points out. This is however too brittle for most work, but by an admixture with silica a glass could no doubt be made which would be perfectly satisfactory. If some glass manufacturers would take up this question and furnish us such a material for electrical instruments, the greater part of the present annoyance met with in making delicate experiments would vanish. It would not leak, would show dirt, could be readily cleaned, and would be free from one of the great disadvantages of rubber, *i. e.* a large coefficient of expansion, which is always making trouble by bending terminals of resistance coils, thus changing their value and sometimes opening the circuit.

It is also probable that a fine grade of porcelain would be a great benefit to the electrical profession, if coated with a good non-alkaline glaze.

For insulating the coils of resistances it is doubtful if we have any good solid material. For paraffin cannot be used, as its expansion and contraction are so great that large pressures are put upon the wire and the resultant strains change the resistance. It might be easy enough to prevent the strain on the first solidification in a way similar to that devised by Rowland for cementing flat mirrors without buckling them, *i.e.*, by mixing a little glycerine with the beeswax; the glycerine not dissolving in the beeswax makes it act like a viscous fluid, *i. e.*, deform under the

action of infinitesimal forces in time. The glycerine however finally works its way out like zinc in a resistance alloy, (as first pointed out by Mr. Weston), and if a similar method were used with the coils, it would still be subjected to strains on change of temperature. Another objection which has been made in England is that paraffin absorbs moisture. It is possible that this is due to the dissimilar methods of producing American and English paraffin as I have never had to complain of this, except of course when cold paraffin was placed in saturated moist air. The insulation resistance of paraffin seems however to be markedly increased by the treatment mentioned below. The great objection to paraffin is its tendency to collect dust. Shellac has been recommended, and since the coils are in the dark the material will oxidize but slowly, and if care be taken to use pure alcohol for a solvent, and not denaturized spirit, (which sometimes contains conducting impurities) has a very high resistance when dry. Some forms of Japan lac seem to remain flexible permanently, as for instance the sample *a* (composition unknown) which is ten years old.

Oil is sometimes used for resistance coils, and this is without doubt the best method, since the great point in the use of resistance coils is to know their temperature. The writer's experience with manganin and constantin as practical laboratory standards has been unfortunate and he has hence decided to use only standards of pure lead run into glass tubes and kept in water. The reason is that, other things being equal, the most sensitive Wheatstone bridge is that which takes the greatest current without appreciable heating, and in the ordinary form of resistance coil a very small current will heat the interior up to such a temperature as to alter the value. Moreover, if the coil is of a material not affected by such changes of temperature, it (with our present alloys) will have a larger temperature coefficient, and as the temperature of the interior of the coil is not known this introduces another uncertainty. With the oil mounting, however, this is all done away with, and pure oil has a very high resistance for low voltages.

For condensers and induction coils it is not only necessary to have materials of great ohmic resistance and of great dielectric strength, they must also be perfectly pure and free from admixture. For the first two properties there is nothing so good as paraffin, when properly used, all compositions such as beeswax,

(cerotic acid), etc., being quite inferior in both respects. Paraffin, and what is practically the same thing, pure ozokerite, will stand, according to the tests of Mr. Chesney, which I had the pleasure of witnessing, at the rate of 500,000 volts per inch. This I have confirmed up to 60,000 volts, alternating. Most substances, such for instance as glass, are at once cut out from consideration from the fact that they have too much electrical absorbtion, and heat when subjected to a fluctuating voltage.

We must have an electrically homogenous dielectric, *i. e.* one of the same specific inductive capacity all through. This is for two reasons. First, because, if we have a dielectric A between two charged conductors, the introduction of a dielectric of greater specific inductive capacity, even if of infinite dielectric strength and ohmic resistance, will cause A to break down. To take a numerical case,—suppose we have two plates, 1 cm. apart, and attached to the terminals of a 10,000 volt A. C. dynamo. (Fig. 5.) Suppose the dielectric, air, to support 50 per cent. more than this pressure. Introduce two plates of glass of $K = 8$, each $\frac{1}{4}$ cm. in thickness. Since the voltage divides itself up inversely to the capacitance, we will now have 8,889 volts between C and D. This being at a rate of 17,778 volts per cm. and as it only supports 15,000, we will get a spark between C and D at every reversal of the voltage, which will quickly heat the glass and make it conduct. The full potential of 10,000 will then be between C and D, and a regular arc will form. Thus we see that the introduction of a good insulator will, in all cases where an intermittent or alternating voltage is used, have the paradoxical effect of weakening the insulation, unless the whole space is filled up with the material. This weakening is not generally apparent at once, as the spark takes some time to eat its way back, and this explains why many induction coils only last for a few years of operation.

Another cause is that treated of by Poisson, Clausius and Maxwell.[1] This is, that layers of dielectrics of different capacities and resistances show electrial absorbtion, and this theory has been proven experimentally by Muraoka, who showed that by taking two fluids, neither of which showed absorbtion, a layer of one on top of the other did do so. Maxwell treated the general case. It has, however, been treated in a more specialized way by A. Hess in *La Lumiere Electrique*, Nov. 26, 1892. In this paper are brought out the following points:—

1. "Elec. and Mag.," vol. 1, chapter x.

1. A dielectric, as in Fig. 6, containing conducting particles of water, for instance, may be considered as an arrangement of condensers and resistances in series and shunt with each other. Two cases, shown in Figs. 7 and 8, are worked out, and Fig. 9 gives the curve of charge in the cases of Fig. 7; E is the voltage on the condenser part, and E′ that on the condenser and resistance.

2. A condenser can show large residual charge, though its true ohmic resistance is infinite.

3. With dielectrics showing absorbtion, there will be found some discharge time at which the amount of discharge will be constant at all temperatures.

4. Why in some tests insulation seems to be lower with higher voltages.

5. Why the presence of conducting particles increases apparent capacity.

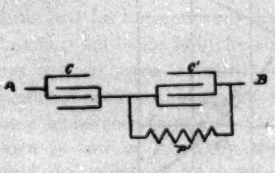

FIG. 7.

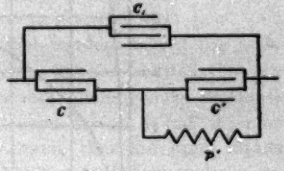

FIG. 8.

6. That to get true ohmic resistance of most dielectrics, voltage must be kept on for a long time, even for days.

7. Why Siemens' method of taking the rate of loss of charge by electrometer does not give correct results.

8. That specific inductive capacity of such dielectrics can only be determined by rapidly alternating currents. This possibly explains an effect noticed by the writer many years ago, *i. e.* that an A. C. static wattmeter immersed in water did not give anything like the torque it should have if the true value of K for water were 80.

9. The importance of getting out the last traces of water in gutta percha and paper when used for cables.

As mentioned, this author considered a simple case of Maxwell's general theory and proved the above results by making actual measurements on condensers and resistances connected up so as to correspond to a simple case of a dielectric of high resistance with conducting particles in it. This paper should be read

by all electricians, especially those concerned with cable work. I would like to speak of this subject more in detail, but for lack of time will only add that most of the conclusions in that paper have been confirmed by me, and that some which had been arrived at independently were seen to be in perfect agreement.

It is this absorption and the consequent losses which make glass useless as an insulator against high A. C. voltages. In some experiments made by Messrs. Stanley and Chesney which were

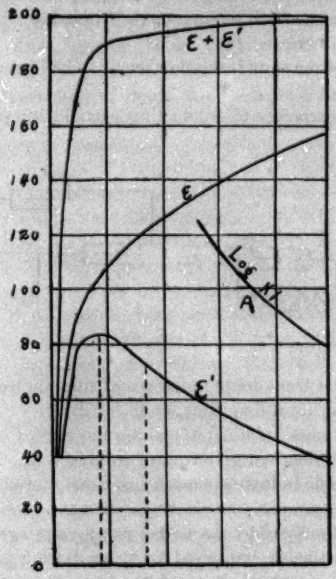

FIG. 9.

shown me, the glass plates of the condenser when on an A. C. voltage, (though thick enough to have stood ten times the D. C. voltage) after a few moments got hot, sparks could be seen passing inside the glass, and the plates finally broke down. Glass is not homogenous, as it is made up of a number of substances, some much better conductors than others, and of different capacities and all stirred together but not dissolved. This is shown by the

care which has to be used in getting glass homogenous enough for optical purposes, if even having, as has been told me by Mr. Brashear, to be kept perfectly horizontal when annealing, as the heavier parts tend so much to sink down to the bottom, even when the glass is only plastic, that the only way to do is to keep the levels of different density parallel to the surface of the disc so that their effect on the light will be as equal as possible for all rays. Otherwise one side of the lens would be of heavy glass and the other light, while at present it is so arranged that one face is dense and the other light. Mica is much less objectionable, especially if its cracks are filled up and it is well dried. Paraffin when properly treated makes very good condensers. The old method of piling together pieces of paraffin paper and tin foil and then pressing them, left much air and moisture inside. This produced large electrical absorbtion and gave large capacity. Messrs. Hutin and La Blanche were the first to discover that good condensers could be made by heating such condensers till the moisture and air were expelled. Their results[1] showed that the specific inductive capacity of this more homogenous dielectric could be reduced from 8 to 2.5. I have myself found it come down as low as 2. They then found that the same results could be obtained by heating the paper before making up the condenser. Since then, this method of forming condensers by heating them to expel moisture, air and acid has been used quite generally, with some modifications and improvements resulting in a shortening of the process.

It may be said as a general rule that the capacity of all substances showing absorbtion may be reduced by this treatment, if the heating be kept up long enough. A great many oils, for instance, are given high capacities, but I have found that in many case this can be greatly reduced by this method, and that the slight remaining excess of K. over that called for by Maxwell's theory can be almost entirely removed by removing the free fatty acids, mucins, etc. Oils tested by me were olive, castor, linseed and cottonseed. All these have very high insulation resistance and low specific capacity when so treated and purified but they soon lose this again when exposed to air. It is evident, therefore, that the anomalous results obtained by Hopkinson and others

1. *La Lumiere Elec;* July 25, 1891.

were due, in some cases at least, to impure material, and such results must be considered as forming a strong proof of the correctness of Maxwell's theory.[1]

But when the substances are not themselves solid, but viscous, they must have a mechanical backing. For this pure cellulose is generally used. Pure cellulose contains some loosely combined moisture. Consequently it can exist in two states. Dried below 100° C. it decreases its specific inductive capacity very much, and has very high resistance and is flexible. Kept above 100° C. for any length of time it loses some of its combined water, has a much higher ohmic resistance and its specific inductive capacity sinks to 1.9 or 2. It however becomes very brittle, and even though the temperature be only a few degrees above 100 C. it finally cannot be bent without breaking.[2] (This brittleness must be carefully distinguished from the so called rottenness which cotton fabrics get when dipped in linseed oil and dried. The fact that cotton tears easily in such a condition is due to the same cause as makes a wire mosquito netting tear when painted, *i. e.* the fibres are stuck fast by the varnish and cannot help one another. This can be proven by removing the dried oil when the fibre will be found to have nearly its original strength.) In this condition it is best suited for making condensers. The paraffin itself is greatly improved, as was pointed out by Hutin and Lablanc, by heating to about 140 C. Three hours heating I have found satisfactory. The dried paper, immediately on removal from the oven, is plunged into the hot paraffin, so as to protect it from absorbing moisture. The condenser is then made up and boiled so as to remove the air, for several hours. This boiling method was described in a recent patent as a novelty, but it was used by Mr. Chesney at Pittsfield in 1891.

A condenser so made, if perfectly pure cellulose is used (perfectly pure paper is used in practice), and with pure paraffin, will stand 250 volts per thousandth of an inch when the dielectric is

1. A rule connecting this effect with the sign of the Kerr's electrostatic optical effect has been given by the writer.—*Elec. World*, Jan. 2, 1897.

2. It is for this reason that electric apparatus should never run above 212° F. As engine rooms sometimes reach 130° F., this means a limit of 82° F. or 45° C. above the air. But when this is taken with thermometer, the rise indicated by the thermometer should be much less. With the introduction of high speed rotary engines we may expect to see much higher field strengths, hotter armatures and different insulation, say armatures with copper bars enamelled in the iron sheets.

less than .01 inch, and at a higher rate for greater thicknesses, when the effect of small defects in one sheet of paper is not so serious.

Practically the same remarks apply to the making of induction coils. Here, however, we meet with the great difficulty that the paraffin in cooling is sure to shrink, and will leave hollows inside. The way to get over this is to construct the coil so that when cooling the shrinkage will take place outside, just as if one were making a casting of some metal having great contraction. The coefficient of adhesion also should be less between the walls of the mould and paraffin, than between the wire and paraffin; also the outside should never be let harden first, as then, of course, a hollow space is left inside. Another precaution is to expel all gases by heating the paraffin for some time above the temperature at which the coils are to be boiled. The coils should be boiled above 100° C. for some hours to drive off the loosely combined water. This destroys the mechanical strength of the cellulose, but as the whole coil forms a solid mass this is of no great consequence.

Silk should never be used where high insulation is required, as pure cellulose dry and boiled in paraffin is so much superior to it that there is no comparison. With pure cellulose, coils with only 1700 feet of wire per inch of spark stand perfectly, *i. e.* the spark may be five times longer than the coil, In ordinary use, coils having a spark length $3\frac{1}{3}$ times that of the coil have been run for long periods with no break-downs.

As regards oil insulation for ordinary induction coils, the writer has not had sufficient practical experience. I believe, however, that very good results are obtained. With regard to the Thomson high-frequency coil,[1] there is no question of the efficacy of oil there, especially with regard to ease of repair. As is well known, however, the oil and coil should always be heated above 110° C. for some time if the best results are to be obtained. A very curious increase of insulating power for high frequencies in oil has been noted by Elihu Thomson, who has suggested that it might be due to inertia of the molecules of oil. To test this, one of the writer's students, Mr. Bennet, constructed a two-phase high-frequency electrostatic field. Though insulators placed in this rotated even when placed within a $\frac{1}{16}$ inch glass flask, the

1. Popularly known as the Tesla coil, on account of his having brought it into prominence through his use of it in his lectures; though it was invented and first described by Elihu Thomson.

effect was found to be due to air currents, and when these were eliminated no movement was obtained, so that the cause of this effect is still unknown, and it it doubtful if true dielectric hysteresis has ever been observed.[1]

So far as Mr. Chattock's experiments and the experience of the writer go, there is not a great deal of difference between the dielectric strength of different substances. It is probably related to the tensile strength. As mentioned previously, the writer[2] pointed out that there was strong evidence to show that the tensile strength of a substance was due to the mutual attraction of charges on the atoms and that the observed values agreed well with the calculated and followed the same law. Some time later Chattock[3] in a very interesting and able paper showed that, as the results of his experiments, the dielectric broke down when the slope of potential was great enough to pull apart atoms having charges of the same dimensions as the ionic charges. This was shown for gases, fluids and solids, and forms a very interesting—and, I believe, independent—corroboration of the writer's electrostatic theory of cohesion. Consequently, the nearer the atoms are together and the greater their rigidity, the greater also their dielectric strength.

Chattock's experiments give for solids and fluids—

SUBSTANCE	VOLTS PER CM. FOR BREAKING DOWN.
Glass	919.000
Water	1.050.000
Oil	930.000

which agrees well with the theory.

From the above it will be seen that if the materials are pure, and ohmic resistance is of not much importance, a compound having its molecules held together tightly will have a good dielectric strength for D. C. voltages.

This paper is already so long that I cannot touch in detail on the question of cables. There are also other papers in existence written by men better equipped for the task. I had intended saying something about what Siemens has called the "absurd

1. Righi some time ago published a paper in which he claimed to have observed dielectric hysteris, and gave a law for its amount. Since Mr. Bennet made his experiments, however, Righi has retracted his statements, having found the effect due to other causes.

2. *Elec. World*, Aug. 8 and 22, 1891.

3. *Phil. Mag.*, Dec., 1892.

craze for high insulation resistance," but the fact is now generally recognized, except by inexperienced engineers, that the best cables are those of medium ohmic resistance. I will only mention two methods which have occurred to me as feasible for certain purposes. One is based on the fact that the dielectric strength of air, as shown by the experiments of J. J. Thomson and Peace increases very rapidly with the pressure at 90 lbs. per sq. inch being equal to that of a good quality of rubber. A similar plan, though not requiring any very large pressure, is due to Mr. Westinghouse, who thought of employing it four or five years ago in Philadelphia. The second occurred to the writer on reading Elihu Thomson's article on the use of liquid air as an insulator. It is this: Since ice at only 12 below freezing has a specific resistance of over 1000 meghoms, *i. e.* as good as some brands of insulation, why not make the conductors hollow, lay them in a trench filled with water, pass cold brine through the pipes, use the brine for cooling houses, making ice etc., and let the frozen water act as the insulator. A rough calculation shows that this is commercially feasible, even neglecting all sources of profit from the furnishing of the brine, (*i. e.* if it were used only for cooling the pipes.) After making all allowance for friction of fluid, cost of power, etc., the balance comes at the right end, if the line is always fully loaded.

The question is sometimes raised, whether we can ever hope to have a non-inflammable substance which shall be elastic like india rubber. The probable cause of the elasticity of rubber is known, [1] and it would seem as if there was no reason why such a substance should not be prepared. All we have to do is to coagulate one substance in the midst of another. In fact we have at present in tetrametaphosphate of sodium such a substance, elastic as rubber, transparent and tough, and when pure a good insulator. It would be an admirable material if it were not for the fact that the elasticity is due to water, and when this dries out it becomes brittle.

As regards an organic artificial rubber, I have very little doubt but that it will be made as soon as it is understood by chemists that its properties are due to structural and not chemical causes.

Armature windings.—The present methods of using mica leave little to be desired. The writer might mention, however, one novel method he used in a case where very heavy currents

1. Molecular Physics, Franklin Inst., Sept. 18, 1896.

were to be carried. Asbestos and silicate of soda, as is well known, form a good coating, but is however, poor mechanically. The armature bars were wrapped with asbestos string and then coated with the silicate. This made when dry an extremely firm covering which could only be removed with a hammer. Though at first a bank of 100 lamps could be lit up through the insulation, after a little running it dried out to quite a high figure and the machine did good service, at one time running several hours as I am informed on good authority, under such an overload that the carbon brushes were red hot.

In cases where cloth is to be treated we have a very different question. There are two ways of using cloth, 1st, as a backing merely, by coating it on the surface with some substance which is supported by it, as plaster on lathing. Many substances work well in this situation but the fact that little tubes of cellulose are very apt to stick up through the coating, as was pointed out to me by Mr. F. R. Upton many years ago, and that if moisture gets in at the edge it spreads all over, renders it not the best kind of insulation. Rubber is sometimes applied in this way to cotton tape, but though of very high resistance and insulation at first it rapidly deteriorates. In general it should be said, that where a permanent result is desired rubber should never be used unless kept in the dark and out of contact with air. If these precautions be neglected the life is very short. The other method is to saturate the whole cloth with some substance which will penetrate every crevice, but when this impregnating substance has solidified it must continue to fill these crevices and capilliary tubes. For this reason no substance which is dissolved in anything else can be used. If for instance we try a varnish dissolved in alcohol, it will be found that the strength of the solution in the capilliary tubes is much smaller than outside, for the same reason that sea water filtered through sand becomes fresh.[1] Consequently on drying these capilliary spaces are not filled up and let water in. Therefore unless we adopt the first method and plaster the insulator on thickly and deep enough so that it does not matter whether the support insulates or not, we must use melted solids or drying oils. Unfortunately but few solids which melt are *elastic*, since this elasticity is obtained by a structure which is destroyed by melting, and those solids which melt into thin liquids and remain *flexible* when solid do not preserve this property ex-

1. J. J. Thomson, "App. on Dyn. to Phys. and Chem.," p. 190.

cept within narrow limits of temperature, as can be easily tested by holding under a cold water tap and striking the specimen sharply. Soft paraffin can be used in some cases if the cellulose be well dried and thoroughly saturated. The asphalts cannot as a rule be used, as they never get sufficiently fluid on melting. There is, however, one notable exception: uintaite or, as it is commercially called, gilsonite. This substance I found many years ago had the peculiar property that, when melted, like paraffin or oil, it will pass into the pores of cellulose or cloth. Having a very high melting point, nearly 300° if I ermember, and mixing perfectly with paraffin in all proportions, it gives mixtures which are admirably adapted for induction coil work as these compounds can be made to have high melting points and to penetrate a coil thoroughly. I also some years later, in 1891, used this material in combination with linseed oil for transformers, the process at first proposed being boiling in vacuum, but it was found that even without this saturation was complete. I understand that this method is still used, though modified in form, by the company for which I first devised it. Of the drying oils, with the exception of some foreign oils as Chinese wood oil, and an African oil whose name I cannot recollect or ascertain, linseed and the drying nut oils are the best. Linseed oil has the remarkable property of expanding on drying. This enables it to fill up all pores. Its durability is evinced by the good condition of old oil paintings. The varnishes crack and go, but the oil remains. Its insulation is not injured up to very high temperatures at which shellac, rubber, etc., would be worthless This material was used a great deal by the Edison company in its early days, but it often broke down. The trouble was traced to the lead drier, and after many experiments Mr. Marshall who had charge of this work finally settled upon the use of pure raw oil. This gave excellent results and was long used but took some time to dry, and the writer finally, after many tests found that borate of manganese drier got rid of the trouble, while as is well known, it gives a very quick drying varnish. This was used by the United States company in Newark on their machines, with the result that in 1890 after use for a year, the foreman reported only two armatures so treated as returned for repair, (they were injured by lightning) and no fields. This material was also used by the Stanley company for transformers. Another advantage of this borated oil is that it always retains a slight stickiness, and

so gives a good joint when wrapping around wires, etc. Many substances so used are not sticky and let moisture in through the joints. Where a smooth surface is required, it is readily obtained by dusting on a little talc, a method first suggested, I believe, by Mr. Edison. It can also be given a coat of Japan on the outside. Varnish gums should never be used with linseed oil, as they are brittle, and the dried oil is only just flexible enough. Consequently when the oil has dried the resultant varnish is always very brittle. A temporary elasticity is given at first by the fact that when the solvent has dried off the oil is still fluid and undried, and as the varnish gum keeps the air from getting at it rapidly, it sometimes remains flexible for a year. Such mixtures also crack when cold.

Sample *c* is a specimen of borated oil saturated cloth, which is now between eight and nine years old. It will be noted that it is still fresh and flexible, and a recent dielectric strength test showed up very high, 15,000 volts if I recollect. The pure raw oil is boiled at about 200° with ½ per cent. of borate of manganese for several hours till it begins to be thick.

Non-inflammmable materials can be made, as I have pointed out elsewhere, by taking out the hydrogen atoms of hydrocarbons and substituting chlorine. Even paraffin can be thus treated if kept warm, and first turns to a fluid and then to a solid. At one time it seemed as if this process might be valuable, but the use of enclosed conduits has done away with the greatest source of danger from fire.

I will conclude by describing a couple of devices which I have found useful in preventing insulation from being spoiled. Soldering acid, as commonly used, is a solution of chloride of zinc. If this falls on cellulose it turns it to a paste. It never evaporates and always takes up moisture from the air, and will gradually eat its way through quite a thickness of insulation. Whether it is acid or neutral makes no difference so far as its action on the insulation is concerned, though the neutral solution does not corrode the wire. Rosin has the disadvantage that it is not a fluid and is clumsy to handle. I have found that by shaking up powdered rosin in very strong ammonia, an ammonia soap is produced which works well in most cases. The ammonia dissolves the copper oxide and evaporates afterwards, leaving the powdered rosin, which is an insulator.

Apparatus can be protected from overheating by putting in

the apparatus a small glass tube filled with carnauba wax. This melts near the danger point, but remains quite hard up till then, so that by imbedding a spring and contact in the wax, when the apparatus gets too warm the wax gives, and the spring expanding causes a short circuit which blows the fuse.

The largeness of the subject must be my excuse for the fragmentary nature of this paper. After I had begun it, I found I had made a mistake; what I should have undertaken was to write a book. I trust, however, that some of the points I have developed may prove of interest.

DISCUSSION.

THE PRESIDENT:—I can only repeat what I said before, that it is most unfortunate that the author is unable to be present. It strikes me that there is no very direct connection between the first part of the paper, which is theoretical, and the last part which is extremely practical. I fail to see anything but a connection in name between the two. But there are a number of very significant points in the theoretical portion and a number of practical ideas in the second part, based on considerable experience. The first part seems to be somewhat empirical, a sort of a groping, as it were, for some formula which would give an indication of the conductivity of substances. But that is always the way that science progresses, by laying down somewhat empirical laws which are afterward more clearly understood and appreciated. There certainly are a number of important physical facts that are almost within sight and many of them would appear to be more or less touched by these formulæ.

I believe Prof. Franklin had read this paper and considered it before the meeting. I will call upon him to discuss it.

PROF. W. S. FRANKLIN:—I would say, Mr. President, that my estimate is very much as you have expressed yourself in regard to the relation between the first and second parts of this paper. I am very much impressed with the desire of Professor Fessenden to arrive at some theory which will enable him to classify and systematize our knowledge of the phenomena of conduction and so on, and a great deal of the matter in the first part of the paper is entirely new to me. There is one thing, however, which I would like to say in connection with the molecular theory in its application to physics. You will notice that the first part of the paper is based wholly upon the molecular theory, and what I wish to say is this, that the most active part of physics at the present time, namely physical chemistry, has practically discarded the molecular theory altogether. You never hear a physical chemist talking about molecules unless he

is finding fault with somebody else who has been talking about them. The only things that physical chemists tolerate in the fundamentals of the subject are those which can be either felt or seen or taken hold of or measured, and I have at times thought that this tendency will gain the upper hand, and that we will in time drop the molecular theory entirely. I think it would be a very good thing if it could be dropped. Notwithstanding that fact, it seems to me that the author has shown some very remarkable relations between the mechanical properties of substances and their electrical properties by the help of the molecular theory.

In connection with the matter which is discussed in the opening paragraph, namely the two distinct properties of an insulator, there is one quite striking illustration of that which perhaps may be of interest.

Electrical insulation, or the prevention of electrical motion, is analagous to the mechanical operation of fixing bodies by ropes and beams to prevent their motion; and, considering the materials which are used as insulators, it is very much as if we had only molasses or pitch to make our ropes and beams of. If we had only pitch to make ropes of, there would be two very distinct items of strength. If you wished to subject a pitch rope to a given stress you would need to consider: First, at what rate the rope would lengthen. This rate of lengthening multiplied by the stretching force would give the rate at which energy would be continuously dissipated in the stretched rope.

Second, whether the given stress would be sufficient to snap the rope in two.

We have thus a complete mechanical analogy of the two items of strength of insulators. In one case the insulator gives way continuously under the electrical stress and in the other case it breaks to pieces.

There is one other point in this first part, which, as it occurs to me, might well be mentioned, and that is the relation between the conduction of sound or the transmission of sound in substances and that of electricity. Of course when I first read the paper, it struck me that the two things were so entirely different that you could not conceive of any relation between them at all. Sound has a definite velocity, while we all know that electricity has no definite velocity through a substance. But that really is not an essential difficulty when you think about it, for this following reason:

In the first place the transmission of sound through a substance is essentially identical to the action which we call stationary waves, so that the conception of velocity of transmission is not inseparably connected with the mechanical movements which we call sound. In the second place the enormous rapidity with which an electric current spreads along a wire is known to depend upon wave motion (ether waves) in the medium surrounding the

wire, while the movements at a point in a wire which constitute the conduction of the current at that point may be sluggish movements of the small parts of the wire itself of a character similar to the motion which constitutes stationary sound waves.

I am not able to say much concerning the practical part of this paper except that I have been very much interested in it, and that it seems to me to show that the author has had a great deal of actual experience in the matter of the properties of insulators. I may say however that I have had considerable experience in the making of galvanometer coils using paraffin, and boiling in vacuum, and this method seems to get rid of moisture so much better than any method that I have ever tried that I am surprised to find that Professor Fessenden has tried the method and discarded it. It may of course be unnecessarily complicated for many purposes, but it seems to me that it would be the very best method for treating induction coils and transformers.

THE PRESIDENT:—Is there any further discussion on this subject?

PROF. FRANKLIN:—It seems to me that there are two extremely valuable suggestions that Professor Fessenden makes in this paper, one is in regard to the possible discovery of a kind of glass which would be free from the insulation defects of ordinary glass, and the other is in regard to the great importance of in some way managing it so that a man who wishes to make a test and is willing to put a great deal of time in making an accurate test can get pure material to work on. I suppose every one here has had difficulty in that respect. I myself have put in simply an enormous amount of time in testing, for example, the magnetic properties of iron, and of course as you all know it is almost impossible to know what kind of iron it is you are buying. You get a sample of wire and the man you buy it from does not know anything about it, and the man he buys it from perhaps does not know anything about it, and if you could possibly trace it back through two or three steps you might come to the manufacturer who could perhaps tell you a litile about it but will not take the trouble, as a rule. But the getting of pure materials for the tests which are being carried out in great numbers all over the country seems to me to be a thing which is of extreme importance and Professor Fessenden's remark ought to be taken note of and acted upon in some way or other. It seems strange that we should annul the value of work which is costing the country literally thousands of dollars per day, by neglecting this fundamental matter which would cost almost nothing at all!

DR. PUPIN:—Mr. President, although rather late, I think that a few words should be added to this voluminous paper. I think that the first part, as you yourself have observed, has hardly any connection with the second part. It is unnecessary as far as the appreciation of the second part is concerned. It reminds me very much of the proceedings of a certain number of

ladies forming a literary club One of the members is appointed to deliver a lecture on something which is as a rule very advanced indeed, and very obscure as a rule and very uninteresting. But fortunately it is always relieved by a nice supper afterward and the hostess, as a rule, does not neglect the quality of the supper, because without the supper the lecture would not draw. So scientific writers sometimes who have something very important to communicate, something really very interesting and enjoyable, will preface that by something that is not very interesting or enjoyable.

There is no doubt that many among us have thought a great deal about the purely theoretical side of conduction. There is nothing perhaps in the whole range of the science of electricity which is so little understood as the conduction of current. What is the process that is called the conduction of the electrical current? Who has any definite idea about it? I do not think that Maxwell has ever expressed a definite opinion on the subject at all. In fact, the theory of the mechanism of conduction has never been attempted until within the last few years, and then it was attempted in consequence of the marked advances which have been made in physical chemistry. There is no doubt that the conductivity of a salt solution is very intimately connected with its degree of dissociation, so that if you know the osmotic pressure or the vapor tension depression of a salt solution you can by indirect method calculate its conductivity, starting, of course, from the hypothesis that the conductivity is due to the dissociated molecules. Then a great deal has been done on the conduction of current through rarefied gases by the beautiful experiments of Prof. J. J. Thomson in which he showed that a vacuum tube will act as a screen for electric waves, etc. Now in all these cases we cannot help being led to the conclusion that the mechanism of electrical current consists in the carrying of electrical charges, by the dissociated ions of the gas in one case, and of the salt in the solution in the other case. Whether we have a right to infer from this particular class of conduction that in the case of metallic conductors the conduction takes place by a sort of dissociation or not, remains to be seen. I certainly think that we have no right so far as experimental data at present are concerned. A generalization of the kind was suggested by Prof. J. J. Thomson in his book on "Recent Researches in Electricity and Magnetism." Now whether the electrical mechanism of conduction be a sort of electrolytic convection or not I do not really see how the coarse mechanical properties of a substance, like elasticity and density, can have any direct connection with electrical conduction. To be sure these empirical formulæ which Prof. Fessenden has given are very suggestive. But I do not think we ought to take them *au serieux* They are simply interesting empirical formulæ and not anything else, and I do not think Prof. Fessenden intends that they shall be taken in any

other way than simply as suggestive formulæ. They serve to assist our memory in remembering the electrical properties—the electrical and physical constants of materials.

I cannot say that the author has always been entirely correct in his statements of the opinions and theories of former physicists. It is too late now to go into a close discussion of this matter. But one statement seems to me to be very much out of the way, and that is the statement on page 199 where he says: "The electric conductivities are in the same ratio as the "velocities of very short sound waves, being thus analogous to "Maxwell's law for the velocity of light in insulators, in that "the velocity varies with the periodicity." *In that the velocity varies with the periodicity*—now, as far as I remember, Maxwell has never given that law, and in fact Maxwell's theory is incomplete in this very point, in that it gives the same velocity for all periodicities. It is only due to recent advances in the theory of electricity that the dispersion of light has been explained on the basis of Maxwell's theory. Dispersion of light is possible only if different periodicities travel with different velocities. So that I do not see where Prof. Fessenden has found that in Maxwell's theory. That seems to me to be an entirely erroneous statement.

I think the practical part of the paper is most excellent. It shows that the author has had a very large experience in the handling of various materials, of conductors and non-conductors, and although I think that to most of the electrical engineers who have had considerable experience in research work, all these statements will not be new, still I think it is a very useful to have all these experimental data classified and recorded as something to which we can refer. I think, therefore, that it is a very good thing to have in the TRANSACTIONS a few pages like these where valuable suggestions concerning the properties of materials are recorded in a systematic way. For that part of the paper most of us, I think, feel as I do, that we ought to be very grateful to Prof. Fessenden.

[Adjourned.]

[COMMUNICATED AFTER ADJOURNMENT BY REGINALD A. FESSENDEN.]

I regret that an important business meeting rendered it impossible for me to personally present this paper before the INSTITUTE, but I take the opportunity to touch on some of the remarks made during the discussion.

First, with respect to the connection between the different parts of the paper, I would say that this is much more close than appears to have been appreciated; in fact, it is almost too obvious to require mentioning that a knowledge of the theory of insulation requires a knowledge of the theory of conduction. Insula-

tion is one of the most important points in electrical design, but I have no hesitation in saying that there are not more than one or two companies in the United States whose insulations are worth more than so much bare cotton cloth after two years' service. By this I mean that if a piece of apparatus be taken after one or two years' use and put in a box, and the bottom of the box flooded with water, the insulation resistance of the apparatus will begin to go down and in a short time it will fail to pass a voltage test, thus showing that the insulation is no insulation at all, properly speaking, but merely a mechanical means for keeping the wires apart and insulating only just as much as a piece of cotton tape would. To take one example, I had recently sent me by a large manufacturer of street railway motors a piece of insulating material for which he claimed very considerable virtues; which had been the best insulation he had used so far and which he had only abandoned on account of its very high price. On my testing a sample taken from a motor about a year old I found that it had been originally a very good quality of canvas, covered, above all things in the world, with rubber. Of course, the rubber had totally gone to pieces and was in the form of a black non-cohesive powder. Its insulating properties proved so poor that on placing a drop of water on one side of it, in 30 seconds it came through on the other side.

The trouble is that the insulating department of a company is generally put in charge of some young man of very little practical experience. He is generally a good and able man, makes a lot of experiments which have been made over and over again by others, but not published; thinks he has found some good things, and after a year or so, when he begins to see how the machines brought in for repair look and is really beginning to learn something, is promoted to a better position, and the next man goes through the same thing. To again take an example, one of the most common things for the beginner to do is to make up a gum varnish as an insulator. I know personally at least ten cases of this. Now, old hands know that a gum varnish is no good, that it cracks within a year, that it splits in cold weather, that it never makes a good joint when used on paper or cloth, and that its only virtue is that it looks pretty for a time. Yet I suppose that there are at the present time at least a dozen companies using varnishes for insulation, at from $1.50 to $4.00 per gallon, when pure borated oil, superior in every respect, can be got for about 30 cents per gallon.

Now all those details would make a long paper, too long to read at such a meeting as that of the INSTITUTE, and I have therefore merely indicated a general theory of the subject with a few illustrations.

I thoroughly appreciate the kind remarks of Professors Franklin and Pupin. I however disagree with them entirely on certain points. First, with respect to Professor Franklin's

remark about molecules. I have myself some knowledge of chemistry, and was for some years head chemist of Mr. Edison's laboratory. I have also made a specialty for some ten years of physical chemistry, and I have no hesitation in saying that the foundation of almost all good work in chemical physics must be the behavior of the molecule. It is true that there has been a good deal of manipulation of energy equations lately by the chemists, and I will not deny that some good work has been done in this line, but it must not be forgotten that this is only one small branch of physical chemistry, and that the results so obtained can generally be obtained by much simpler methods. I think Professor Franklin will appreciate this if I ask him to predetermine the density of, say, cæsium chloride; to predetermine the tensile strength of magnesium; to predetermine the angles of a crystal whose chemical composition is known, without resorting to other analogous salts; to determine a law for the compression of gases which shall have but one arbitrary constant and shall yet express with more exactness than Van der Waal's equation the pressure volume curves for *all* gases, or to give a strict and satisfying mathematical proof that Dulong and Petit's law is a true physical law and not a coincidence. I think that it will be admitted that this cannot be done without resource to molecular theories, but all the above have been done by the writer, and all published except the last. Many beautiful instances are given by Professor Roberts-Austen in his papers, for instance the predetermination of the tensile strength of alloys. I venture to express the opinion that the recent work of Larmor on combinations of vortex rings, together with the newly discovered fact that the unequal shifting of the lines in Zeeman's effect must depend upon the valency of the atom; that the valencies of an atom must be separate and distinct things, as distinct from each other as two interlaced rings; that each valency must give rise to a distinct set of lines, shifting differently to the lines due to another valency, the shifting being given by a simple expression, will give fresh impetus to work in this direction. There is reason to believe that this year will ever be memorable as the date on which we first began to obtain definite information in regard to the structure of the atom.

It is not true that atoms cannot be measured. They have not been measured very accurately at present, but any chemist who should determine the latent heat of vaporization and the surface tension of mercury would be able to determine from them the size of the atoms, and hence that of the molecules, with a degree of precision approaching that of many atomic weights.

I am very much pleased with Professor Franklin's mechanical analogy. It brings out things in a very pretty way. With regard to the question of boiling in vacuo, I would say that Professor Franklin is perfectly right in saying that it is better

than in air, but my experience has been that if the boiling is done as I have described, the difference is very slight. I do not know the reason of this; possibly it is that when the cellulose begins to decompose, it drives out the rest of the air, and when the paraffin cools, the hydrocarbon vapors are absorbed by the paraffin. I have never made any experiments to test this theory, however.

With reference to Prof. Pupin's remarks, I hope he will pardon me if I say that he would never have made the statement that the rate of flow of a current "could not have anything to do with such coarse mechanical properties of matter as elasticity and density," if he were not deeply versed in the writings of the modern mathematical physicists who have treated of electrical problems. I will add, if he were not *too* deeply versed. I have a complaint to make against these gentlemen: they cannot be got to realize that there is a difference between a force and a flux, between H and B for instance. In a paper in the very last *Philosophical Magazine* for example, the formula for the Zeeman effect is given wrongly, H being written throughout for B. As a consequence of this neglect of μ, they out-Maxwell Maxwell, and any one reading their papers would get the impression that, as one of them has actually put it, the only function of a wire was to dissipate energy.

This is going too far. It is entirely false. The dissipation of energy is merely an incident and has nothing to do with the function of a wire, which is to carry what we call the quantity of electricity. Take for instance a copper wire at absolute zero. It then dissipates no energy, but does any one suppose that we could then remove the wire and still have a current? Not unless the ether at absolute zero were a conductor, which is negatived by the fact that the sun's light traverses space, which it could not do if the ether in space were not a perfect insulator. A sensational idea, even in science, is hard to kill, no matter how false; as witness, the often repeated statement that light is due to charges oscillating on the atoms, as in small condensers, though this statement has been refuted time and again, and by the very man who first made it. And so we must be prepared for some years to hear that the ether is the all important thing, whereas, as a matter of fact, it is merely the thing which pushes along the thing which we call the current.

Now we know three ways of producing a current. Convection, electrolytic conduction and metallic conduction. We *know* that in convection the current does not travel with the velocity of light, but that, by putting charged balls in the insulated buckets of a coal conveyor, we may have any current we please, and the electricity traveling as fast or as slow as we please. In electrolytic conduction we have very good reason to believe that the electricity travels on the atoms, and

at a speed of only a few centimetres per second. Having thus one instance in which the current is *known* not to travel with the speed of light, and one in which it is almost certainly known not to do so, does not the burden of proof that the current in a wire does not flow also at some slower rate fall upon these "all ether" physicists?

Now, as to the actual relation given by me, the facts are these: Dr. Lodge, as I have pointed out, many years ago showed that it would be a natural thing if the current flowed at the velocity of sound, but, having no data for the velocity of sound in wires, or rather, incomplete data, stated that there did not seem to be any relation. I, working in an empirical manner, found a formula showing a relation between the velocity for sound and the conductivity. This I published years ago, and gave in it the conductivity of aluminium as 66 per cent. that of copper, the best determination at that time being that it was only 50 per cent.; but as I had forgotten about Lodge's work, I did not refer to it. *Now* the latest determinations of the conductivity of aluminium give it as I had predicted it.

In spite of this, Professor Pupin says that he "does not think that we ought to consider these formulæ *au serieux*." To which I can only say that I conceive Professor Pupin to be possessed of a very happy disposition. To speak seriously, no real advance of importance can be made until we give up this idea of attributing everything to the ether and come back to face the facts. Permeability and specific inductive capacity are real things, even their general forms are known, and we can prove from Faraday's law of electrolysis[1] that at least one of them must be a complex quantity. A great deal of harm has been done by this neglect of the functions of matter, and many students have had their ideas confused by it. Too much has been laid to the ether.

With regard to Professor Pupin's remarks on Maxwell's law, they are perfectly correct, but I never wrote the sentence as he quotes it. The compositor has seen fit to introduce a colon before the word "these" which should not belong there, and as originally written, the nominative of the verb "being" was the word "relation." The sentence thus means that Maxwell's law, $k^{\frac{1}{2}}\,\mu^{\frac{1}{2}}$ is proportional to the slowness of light, and my rule, that $\mu^{\frac{1}{2}}/k^{\frac{1}{2}}$ is proportional to the slowness of sound, have this in common, that the function of k and μ varies with the

1. This was indicated, of course, long ago by some of the optical theories, but it has been shown by Rayleigh and others that this does not give satisfactory results in all cases. The proof depending upon Faraday's law is however unexceptional, though at first sight there would seem to be no immediate connection.

periodicity of the motion. Maxwell, of course, in his law never made any statement that k or μ varied with the periodicity, though he shows that k does in other portions of his work. I would thank Professor Pupin for calling my attention to this slip.

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